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WHERE THERE'S SMOKE:
STOCHASTIC CAREGIVING SHOCKS AND MOTHERS' LABOR MARKET OUTCOMES

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ABSTRACT

Formal childcare and public schooling are widely understood to be central to mothers’ labor market attachment. Yet, a large literature finds that even substantial expansions of formal childcare produce modest or null effects on maternal employment. We study the role of stochastic caregiving demand shocks that cause childcare constraints to bind intermittently even when formal care is available. Using plausibly exogenous variation in wildfire smoke exposure, we show that smoke increases school closures and student absenteeism, generating caregiving demand. Further, by comparing mothers whose youngest child varies in reliance on school-based childcare, we find that these shocks impose substantial contemporaneous costs and, when accumulated, reduce employment. Employment losses are fully offset among mothers working outside male-dominated industries, consistent with workplace tolerance moderating the consequences of intermittent caregiving demands.

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1 Introduction

Formal childcare and public schooling are widely understood to be central to mothers' labor market attachment. Yet a large literature finds that even substantial expansions in access to formal care often produce null effects on maternal employment, especially in developed-country settings (Cascio, 2009; Goux and Maurin, 2010; Fitzpatrick, 2010, 2012; Havnes and Mogstad, 2011; Nollenberger and Rodríguez-Planas, 2015; Kleven et al., 2024).[1] We argue that this puzzle reflects a distinction the existing literature has largely overlooked: the difference between the availability of formal childcare and its reliability. Even when care is nominally available, unexpected disruptions such as school closures or child illness can cause caregiving constraints to bind intermittently. These shocks impose immediate costs, and when they recur, they can alter mothers' assessments of whether employment is worth sustaining. This paper provides the first causal evidence that such stochastic caregiving shocks generate persistent reductions in maternal employment.

The central empirical challenge is that unexpected caregiving disruptions are difficult to observe directly, and observable sources of variation, such as the COVID-19 pandemic or large-scale disruptions (e.g., natural disasters), are often entangled with other determinants of maternal labor supply. We address this challenge by exploiting downwind exposure to wildfire smoke, rather than exposure to wildfire itself, as a source of plausibly exogenous caregiving shocks. With a changing climate, the frequency and intensity of wildfires have risen rapidly in recent years, making wildfire smoke an increasingly important source of ambient particulate pollution (Burke et al., 2021; Wen and Burke, 2022; Borgschulte et al., 2024). Using administrative school-closure records from California and student attendance data from Texas, we provide direct evidence that wildfire smoke generates caregiving disruptions: wildfire-attributable $\text{PM}_{2.5}$ above $3\mu g/m^3$ sharply increases school closures and student absenteeism even on days when schools remain open—a measure that captures child illness not severe enough to generate hospital visits (Currie et al., 2009).

We then combine this variation with data on time use and labor market outcomes to estimate how smoke-driven disruptions to school-based care affect maternal labor supply. To separate the caregiving channel from the direct physiological effects of smoke exposure on work (Graff Zivin and

[1] Gelbach (2002) and Baker et al. (2008) are exceptions, finding 6 and 14.5 percent increases in employment probability among affected mothers. Even so, effects of this magnitude fall well short of restoring pre-childbirth employment levels (Cortés and Pan, 2023).

Neidell, 2012; Hanna and Oliva, 2015; Borgschulte et al., 2024), we implement a triple-difference design that compares mothers whose youngest child is elementary-school age, and therefore most dependent on weekday school-based care, to mothers whose youngest child is either younger or older. Baseline time-use patterns support this comparison: compared to weekends, weekday school hours generate the sharpest drop in caregiving demands for mothers of elementary-school-aged children, making them especially exposed to disruptions in school-based care.

Our analysis yields three main findings. First, wildfire smoke generates immediate caregiving disruptions. In the American Time Use Survey, smoke exposure increases weekday childcare time for mothers of elementary-school-aged children by 63 minutes (17%), but not weekend childcare time, consistent with the effects driven by disruptions to school-based care—whether through school closures or smoke-induced child illness that keeps children home—rather than through a direct effect of smoke on mothers themselves. In the Current Population Survey, one additional smoke day during the survey week increases full-week work absence and reduces hours worked among these mothers.

Second, these disruptions accumulate into lasting labor market consequences. Each additional weekday smoke exposure in the prior twelve months reduces employment; at the sample mean, the implied effect is roughly a 1 percent decline in employment. For a margin on which even large childcare expansions often produce limited employment responses, this magnitude is economically meaningful.[2] Effects are concentrated in full-time work and driven by labor force exit rather than unemployment. It is the frequency of distinct episodes, not just the duration of any one event, that matters most.

Third, the heterogeneity of cumulative effects points away from a simple resource-cost interpretation. Access to informal backup care does not attenuate the employment effect, nor does working in occupations where reducing hours carries a lower wage penalty (Goldin, 2014). The effect is, however, concentrated among mothers in male-dominated industries, where workplaces tend to be less tolerant of unpredictable caregiving-related disruptions (Kleven et al., 2019). Consistent with heterogeneous workplace culture, we find that mothers outside male-dominated industries absorb smoke-driven shocks through intensive margin adjustments—reducing hours and increasing absenteeism during smoke periods—whereas no such adjustment occurs in male-dominated industries. This suggests that

[2] As detailed in Section 5, this magnitude is also comparable to estimates of labor supply responses to air pollution operating through physiological channels.

the operative mechanism is not the cost of managing disruptions per se, but the gradual erosion of the employment relationship in workplaces that penalize work disruptions arising from intermittent caregiving demands.

Related literature. We situate the paper within four related literatures. First, we contribute to the literature on the motherhood penalty in the labor market by identifying a mechanism that existing work has largely overlooked: externally imposed, difficult-to-predict caregiving shocks. Prior work emphasizes rigid job structure and social norms as the central drivers of maternal employment penalties (Goldin, 2014; Goldin and Katz, 2016; Cortés and Pan, 2019; Kleven et al., 2019; Andresen and Nix, 2022; Kleven et al., 2024). A related line of work highlights the role of caregiving-related interruptions in shaping gender gaps in productivity and pay (Cubas et al., 2021; Frech and Maideu-Morera, 2024; Adams et al., 2025; Anstreicher and Jack, 2025). We show that these shocks not only impose contemporaneous costs, but also, when accumulated, lead to longer-run labor market consequences by changing mothers' assessment of whether remaining attached to work is worthwhile—and we are the first to empirically demonstrate that such repeated shocks reduce employment.

Second, we reframe the childcare question. The existing literature treats childcare availability as the operative margin (Gelbach, 2002; Baker et al., 2008; Cascio, 2009; Fitzpatrick, 2010, 2012; Havnes and Mogstad, 2011; Kleven et al., 2024; Gibbs et al., 2025). We show that reliability is a distinct and consequential margin—one that helps explain why expansions in formal care may fail to produce sustained gains in maternal employment.

Third, we extend the literature on pollution and labor supply beyond the physiological channel. Prior work shows that air pollution reduces productivity and hours through workers' own biological responses (Graff Zivin and Neidell, 2012; Hanna and Oliva, 2015; Chang et al., 2016, 2019; Aragón et al., 2017; He et al., 2019; Adhvaryu et al., 2022; Borgschulte et al., 2024). We identify a second pathway: environmental shocks can affect labor supply indirectly, by disrupting the caregiving arrangements that allow mothers to work. This channel may help rationalize why Borgschulte et al. (2024) find negative earnings effects of air pollution in sectors where direct physiological impairment is an unlikely explanation.

Finally, we contribute to the literature on the gendered impacts of environmental shocks. Existing work focuses primarily on developing-country settings where shocks operate through income

(Björkman-Nyqvist, 2013; Corno et al., 2020). We show that even in a developed-country context, environmental disruptions can widen gender gaps, but through a different mechanism—undermining the reliability of institution-based childcare—and that workplace cultures tolerant of unpredictability can substantially buffer this effect. This perspective links the economics of climate adaptation to the organization of childcare and workplace flexibility, two domains that are often studied separately.

The rest of the paper proceeds as follows. Section 2 provides a conceptual framework. Section 3 provides background on the wildfire smoke and details the data sources. Section 4 describes our estimation framework and Section 5 reports results. Section 6 concludes.

2 Conceptual Framework

To guide the empirical analysis, we develop a simple model illustrating how transitory smoke-driven caregiving shocks can generate persistent reductions in maternal labor supply by eroding current job payoffs and shifting expectations about future caregiving burdens.

Smoke disrupts school-based care (e.g., school closures or child illness) and generates caregiving demand that must be met either by the mother herself or through alternative arrangements. Let B_t denote the cumulative caregiving burden realized during period t. A mother employed during period t receives flow payoff $u^E(w, B_t) = w - k(B_t) + a(B_t)$, where w denotes earnings over the period.

The term $k(B_t)$ represents the resource cost borne by the mother or household in managing caregiving shocks during the period. This cost is multifaceted and varies across individuals and occupations, and can arise even for self-employed mothers. It may include pecuniary costs, such as foregone income or emergency childcare expenses, as well as non-pecuniary burdens, such as the mental load of logistics or the social debt incurred when calling in favors from friends and family. The term $a(B_t)$ captures workplace social capital or match quality—the relational and psychological benefits of being in a supportive and collegial work environment—which enhance job satisfaction. Repeated shocks increase resource costs ($k' \geq 0$) and erode workplace social capital ($a' \leq 0$).

Nonemployment yields constant flow payoff $u^U = b$.[3] Let $\beta \in (0, 1)$ denote the discount factor. Then, the value of employment V^E, equals the sum of the current period payoff and the ex-

[3]We abstract from the option value of job search (McCall, 1970), as our empirical findings indicate that smoke-induced separations are primarily driven by transitions out of the labor force rather than into unemployment. Consequently, the outside option is modeled as a state of nonparticipation with a constant flow payoff b.

pected continuation value of employment, as shown in equation (1). Here, expectation is taken over the probability distribution of future caregiving shocks B_{t+1} conditional on the current state B_t. Following Gallagher (2014), we assume mothers form beliefs about future disruptions based on past realizations, placing higher weight on more recent events. The value of nonemployment is $V^U = b + \beta V^U = \frac{b}{1-\beta}$.

$$V^E(w, B_t) = w - k(B_t) + a(B_t) + \beta \mathbb{E}\Big[\max\{V^E(w, B_{t+1}), V^U\} \mid B_t\Big] \tag{1}$$

The mother re-evaluates the employment decision each period and continues to work if and only if surplus is nonnegative ($V^E(w, B_t) - V^U \geq 0$). This defines a separation threshold (i.e., reservation wage) $w^R(B_t)$ implicitly by

$$V^E(w^R(B_t), B_t) = V^U, \tag{2}$$

so that the stay condition can be written as $w \geq w^R(B_t)$.

To understand the impact of higher B_t on job exit, we start by differentiating equation (1) with B_t. For this, let $V(B_{t+1}) \coloneqq \max\{V^E(w, B_{t+1}), V^U\}$ denote the mother's optimized value for each realization of B_{t+1} in the next period.

$$\frac{\partial V^E(w, B_t)}{\partial B_t} = \underbrace{-k'(B_t) + a'(B_t)}_{\text{Current Payoff Channel}} + \underbrace{\beta \frac{\partial}{\partial B_t} \mathbb{E}[V(B_{t+1}) \mid B_t]}_{\text{Expectation Channel}} < 0 \tag{3}$$

Equation (3) indicates that an increase in the caregiving burden reduces the current payoff through higher resource costs and diminished workplace social capital.[4] Further, because the belief process a la Gallagher (2014) implies that a higher current realization B_t shifts the subjective distribution of future shocks B_{t+1} rightward according to first-order stochastic dominance, the continuation value is decreasing in B_t.

The decline in the value of employment translates directly into a higher threshold for staying in

[4] A potential concern is that past shocks are sunk and should matter only through expectations. However, caregiving disruptions may also affect the V^E independent of the belief channel. An analogy is a firm facing repeated negative demand shocks: even if future prospects remain favorable, the firm may exit if its current liquidity is sufficiently depleted. Similarly, repeated caregiving disruptions can deplete a worker's ability to simultaneously manage work and caregiving demands (akin to worker burnout) such that employment becomes unsustainable even if disruptions are likely to be temporary. This logic parallels the (s,S) inventory model, where the history of past shocks affects the timing of discrete actions through their impact on the current state of a stock variable (Scarf, 1960).

the labor force. To see this, we derive $\frac{dw^R(B_t)}{dB_t}$ by invoking the Implicit Function Theorem:

$$\frac{dw^R(B_t)}{dB_t} = -\frac{\partial V^E/\partial B_t}{\partial V^E/\partial w} > 0 \tag{4}$$

Because marginal utility of income $\partial V^E/\partial w$ is positive, the separation threshold $w^R(B_t)$ is strictly increasing in B_t. This implies that mothers require a higher market wage to remain employed as the caregiving burden accumulates, making a separation more likely.

Theory to data. Let wages be distributed according to the cumulative distribution function F_W with density f_W, where we assume F_W is smooth so that $f_W(w^R(B)) > 0$ in a neighborhood of the separation threshold. The probability of employment conditional on caregiving shocks B is $p(B) = \Pr(w \geq w^R(B)) = 1 - F_W(w^R(B))$ and differentiating yields $\frac{\partial p}{\partial B} = -f_W(w^R(B))\frac{\partial w^R}{\partial B}$. In the model, B denotes recent wildfire smoke-driven caregiving burden. In the data, we use N, the number of smoke days in the past 12 months, as an observable measure of exposure that generates caregiving burden B. This rolling window reflects the idea that the value of employment V^E depends primarily on recent caregiving strain rather than more distant shocks. In Section 5, we show that exposures beyond the past year have negligible effects on employment, supporting this empirical specification. Applying the chain rule yields:

$$\frac{\partial p}{\partial N} = -f_W(w^R(B))\frac{\partial w^R}{\partial B}\frac{\partial B}{\partial N}. \tag{5}$$

Equation (5) motivates our three empirical exercises. First, we test whether smoke exposure increases caregiving burden, namely if $\frac{\partial B}{\partial N} > 0$ by estimating its impact on school closure, student attendance, employed mothers' time use and contemporaneous labor supply. Second, we test whether repeated exposure to wildfire smoke reduces mothers' employment probability, i.e., $\frac{\partial p}{\partial N} < 0$. Notably, although neither the caregiving burden B nor the reservation wage w^R is directly observable, combining the two exercises allows us to indirectly test the key theoretical prediction $\frac{\partial w^R}{\partial B} > 0$: given $f_W(w^R(B)) > 0$, if $\frac{\partial B}{\partial N} > 0$ and $\frac{\partial p}{\partial N} < 0$, then $\frac{\partial w^R}{\partial B}$ must be positive. Third, we investigate the determinants of $\frac{\partial w^R}{\partial B}$. This last point merits further discussion.

To understand which job and worker characteristics better absorb stochastic caregiving shocks, we focus on heterogeneity in $\frac{\partial w^R}{\partial B}$. For this purpose, we approximate equation (4) by assuming $\frac{\partial V^E}{\partial w} \approx 1$

(e.g., under local linearity in wages) and focus on the flow utility representation, which yields:

$$\frac{\partial w^R}{\partial B} \approx k' - a'. \tag{6}$$

Equation (6) highlights two distinct sources of resilience to the shock. The first operates through the marginal resource cost of managing caregiving shock, k', which depends on factors like wage penalty for reducing working hours (Goldin, 2014) and access to affordable backup care, including within-household buffers. The second operates through the marginal erosion of workplace social capital, $-a'$, reflecting how rapidly work disruptions arising from caregiving shocks strain the employment relationship. While we do not directly observe these primitives, our empirical design allows us to assess their relative importance by examining how variation in factors that plausibly shift each margin moderates the impact of caregiving shocks on employment.

3 Background and Data

3.1 Wildfire Smoke Trends and Impacts

The frequency and intensity of wildfires have increased rapidly due to a changing climate, and in recent years, wildfire emissions have accounted for up to 25% of total $PM_{2.5}$ (particulate matter with aerodynamic diameter $< 2.5\mu$m) nationwide (Abatzoglou and Williams, 2016; Burke et al., 2021; Juang et al., 2022). Although wildfire activity is most pronounced in the western US, as Figure 1 illustrates, the impacts of smoke are national in scope due to the long-range transport of emissions from large fires (Burke et al., 2021). Moreover, during most of our sample period, wildfire smoke forecasting tools were limited and largely experimental, with operational high-resolution forecasts becoming widely available only after 2020 (Chow et al., 2022), limiting households' ability to anticipate and plan for smoke exposure in advance.

Exposure to wildfire smoke is associated with a wide range of adverse health outcomes such as increased respiratory and cardiovascular morbidity, and premature mortality, and is particularly harmful for children due to their developing respiratory systems (Aguilera et al., 2021; Zhang et al., 2025; Sugrue et al., 2026).

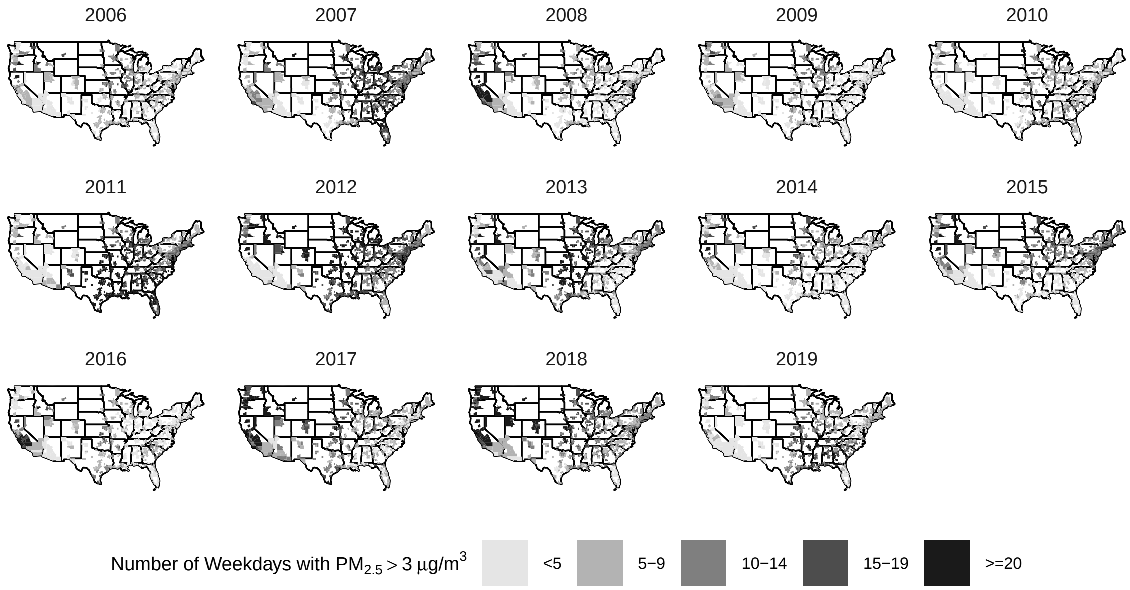


Figure 1: Number of Wildfire Smoke Days by Year. The figure shows the annual number of weekdays with wildfire-attributable $PM_{2.5}$ exceeding $3\mu g/m^3$ for each county. Counties not included in the CPS sample are displayed in white.

Given these health risks, schools often close in response to severe wildfire smoke events.[5] Further, even when schools remain open, smoke exposure can increase illness among children. To measure illness, we use student absenteeism for two reasons. First, as Currie et al. (2009) argue, absence data may be more sensitive to pollution-induced illness than hospital-based measures, since many smoke-related illnesses are not severe enough to generate a hospital visit yet still keep children away from school. Second, absenteeism is a direct trigger of caregiving burden for mothers.

3.2 Data

Wildfire smoke. We utilize daily estimates of wildfire smoke–attributable $PM_{2.5}$ from Childs et al. (2022) for the period 2006–19. Childs et al. (2022) use a machine-learning model to predict wild-

[5]For instance, the California Environmental Protection Agency notes that closing schools during severe wildfire smoke events can protect students by reducing outdoor exposure while traveling to school or moving between buildings on campus, and by preventing heat-related illness when windows must remain closed to keep out smoke (California Environmental Protection Agency, 2019).

fire smoke–attributable $PM_{2.5}$ across the contiguous United States.[6] The model performs well in predicting observed concentrations ($R^2 = 0.67$; (Childs et al., 2022)) and has been widely adopted in recent wildfire smoke studies (Burke et al., 2023; Wen et al., 2023). For the school closure and student attendance analysis, we spatially aggregate the 10km by 10km grid level smoke data to the school district level.[7] For the time use and labor outcomes analysis, we use their county level data.

Labor market outcomes. We use data from the Current Population Survey (CPS), a nationally representative household survey that collects labor market outcomes from roughly 60,000 households (about 100,000 individuals) each month. In each monthly survey, households are interviewed about their work-related activities during the reference week, a 7-day calendar week (Sunday–Saturday) that includes the 12th of the month.[8] We extract the Basic Monthly CPS data from IPUMS for the 2006–19 period (Flood et al., 2025a), restricting the sample to prime-age civilians (ages 25–54) with youngest child aged between 0 and 17.

The CPS provides information on each respondent's labor market outcomes, allowing us to construct various binary indicators for employment status, labor force participation, full-time work, and absenteeism as of the reference week.[9] The data also have a continuous measure of actual hours worked during the reference week. It also contains demographic variables such as sex, age, race, educational attainment, industry, occupation, marital status, and the ages of children. Geographic identifiers in the CPS (as well as American Time Use Survey, which is described below) are typically available at the county level; however, for respondents residing in smaller counties (fewer than 100,000 residents), only Core-Based Statistical Area (CBSA) or state identifiers are provided. For 42% of observations with county identifiers, we match individuals to wildfire smoke exposure at the county level. For 34% of observations with CBSA identifiers only, we assign the population-weighted

[6] They first estimate the relationship between observed $PM_{2.5}$ anomalies (defined as an increases in $PM_{2.5}$ at EPA monitoring stations on wildfire smoke days) and satellite and meteorological data. The estimated model is then used to generate out-of-sample predictions of wildfire smoke–attributable $PM_{2.5}$.

[7] District-level smoke exposure is constructed by matching each district to the grid cell(s) whose centroid falls within the district boundary and averaging across matched cells. Districts without an interior grid centroid are assigned the nearest centroid.

[8] The November and December reference weeks are sometimes moved one week earlier so that survey interviewers are not contacting households during major holiday periods. For November, the reference week will be moved one week earlier if Thanksgiving falls during the week that contains the 19th. For December, if the calendar week including the 5th is contained entirely within the month of December, the December reference week will be one week earlier than normal. For more details, see https://www.bls.gov/cps/definitions.htm#refweek (accessed on Oct 20, 2025).

[9] Absenteeism equals one if an employed individual worked zero hours during the reference week (https://www.bls.gov/cps/absences.htm#atworkPT, accessed on Oct 20, 2025).

average of wildfire smoke level across all counties overlapping that CBSA. Observations identified only at the state level (24%) are removed.[10] Accordingly, while the unit of observation is the individual, the geographic unit at which smoke exposure is measured varies across observations; we refer to this geographic unit as an *area* throughout the paper.

We additionally use CPS Annual Social and Economic Supplement (ASEC) data, which contain retrospective labor-market information for the prior calendar year. Using these data, we construct measures for recent employment exit and entry based on weeks worked during the prior year and current employment status at the March survey.

Time use. We extract American Time Use Survey (ATUS) data via IPUMS to supplement our analysis of labor market outcomes (Flood et al., 2025b). Each ATUS respondent completes a 24-hour time diary for the day preceding the interview, and based on the diary, ATUS documents minute-level information on all activities undertaken during that day for each individual. We merge ATUS and wildfire smoke data at the area by day level.

Following Guryan et al. (2008) and Price and Wasserman (2025), we construct measures of childcare time. Specifically, we define primary childcare time as intervals during which the respondent's main activity is childcare.[11] We then compute total childcare by adding the ATUS measure of secondary childcare, defined as time spent providing childcare concurrently with another primary activity. Importantly, the secondary childcare measure is defined for parents with children under age 13, and thus our ATUS sample is limited to civilians aged 25–54 with youngest child aged between 0 and 12, over the period 2006–19.

To measure smoke-driven disruptions to school-based care, we use two complementary administrative data sources. California school-closure records capture formal school shutdowns, while Texas attendance records capture student absences on days when schools remain open.

School closure. We use administrative records of reported school closures from the California Department of Education, compiled by CalMatters through California Public Records Act requests. The dataset documents, for each school that submitted a Form J-13A waiver to protect attendance-

[10]These shares closely match those reported in Gibson and Shrader (2018).

[11]These activities include basic childcare (e.g., changing diapers), educational childcare (e.g., helping with homework), recreational childcare (e.g., attending a child's sporting event), and travel childcare (e.g., travel related to any of the aforementioned categories). For additional details, see Guryan et al. (2008).

based funding, the dates, reasons, and the district of the school's location for closures. For the empirical analysis, we use the 2006–07 through 2018–19 school years data, aggregate to the school district-day level and merge it with the smoke data at the same spatial and temporal resolution.

Student Attendance. We use school district-level Average Daily Attendance (ADA) data from the Texas Education Agency, covering school years 2006–07 through 2018–19. ADA is defined as the average number of students in attendance per instructional day within a school year (i.e., sum of daily attendance counts divided by the number of days of instruction). A key institutional feature is that formally closed school days are removed from the ADA calculation, and thus ADA captures student absenteeism on otherwise normal school days (Texas Education Agency, 2024).

Summary statistics. Appendix Table A.1 presents summary statistics for the main analysis sample. Panel A reports labor market outcomes for mothers aged 25–54 whose youngest child is between ages 0 and 17. At the time of the CPS interview, 65.3% of these mothers are employed, 4.2% are unemployed, and the remaining 30.5% are out of the labor force. Among employed mothers (65.3%), 49.6% work full time and 15.7% work part time. Slightly less than half of mothers (42.2%) belong to the treated group—those whose youngest child is aged 5–12—while the remaining 57.8% have either younger or older youngest child. The sample experiences an average of 8.6 smoke days (defined as $PM_{2.5} > 3\mu g/m^3$) on weekdays in the preceding 12 months. We focus on weekday smoke events because smoke occurring on non-working days is less likely to have direct labor market impacts.

The last three rows of Panel A report summary statistics for mothers conditional on being employed, which constitute the sample for the contemporaneous effect analyses. Absenteeism—defined as not being present at work for the entire week—averages 4%, and employed mothers work an average of 35 hours per week, while being exposed to wildfire smoke on 0.15 weekdays per week.

Panel B reports variables from school closure and student attendance data. On a given weekday, the probability of experiencing any school closure in California at the school district level is 0.05, while the probability of a smoke day is 0.05. Mean ADA at the school district level in Texas is 39,478, and the average number of weekday smoke days over a school year is 10.4.

Panel C reports time-use measures from the ATUS. Among employed mothers whose youngest child is aged 0–12, average total childcare time is 418 minutes per day, averaged across weekdays and weekends. Approximately one quarter of this time is devoted to primary childcare, with the remaining three quarters classified as secondary childcare. These mothers spend, on average, slightly over five hours per day working, corresponding to roughly 35 hours per week from Panel A. As discussed earlier, because secondary childcare is not defined for children older than age 12, the ATUS sample is restricted to mothers whose youngest child is aged 0–12.

4 Empirical Strategy

4.1 Impact of Wildfire Smoke on School Closure and Student Attendance

School closure in California. We estimate a distributed lag model in equation (7) using the California school closure data to investigate the impact of smoke on school operations.

$$Closure_{zdt} = \sum_{k=-10}^{10} \beta^k Smoke_{z,dt-k} + X_{zdt} + \delta_{zd} + \gamma_{zw} + \theta_t + \epsilon_{zdt} \tag{7}$$

Here, $Closure_{zdt}$ is an indicator equal to 1 if any school in school district z was closed on date dt (e.g., Jan 1, 2015). $Smoke_{z,dt-k}$ is an indicator equal to 1 if a wildfire smoke event occurred in school district z at relative time k, where k denotes the number of days before ($k < 0$) or after ($k > 0$) date dt. Throughout the paper, we define a "smoke event" as a day on which wildfire-attributable $PM_{2.5}$ exceeds $3\mu g/m^3$, which corresponds to the average level of increase in $PM_{2.5}$ observed on wildfire smoke days (Burke et al., 2023). While we control for direct exposure to wildfire by including X_{zdt}, the fraction of school district area that is exposed to wildfire on date dt, it is worth noting that direct fire exposure is rare in practice.[12] Only 0.8% of school district-day observations have any wildfire overlap, confirming that our identifying variation is overwhelmingly driven by downwind smoke transport rather than proximate fire activity. Consistent with this, excluding X_{zdt} has negligible impact on β^k estimates.

In equation (7), we include school district-by-day-of-year fixed effects (δ_{zd}) to control for location-specific seasonal patterns at a fine scale (e.g., Jan 1). School district-by-day-of-week fixed effects (γ_{zw}) account for systematic differences across day of week (e.g., Monday), and school-year fixed

[12] For this, we use California Department of Forestry and Fire Protection's Fire and Resource Assessment Program (FRAP) fire perimeter shapefiles, which include incident start and containment dates along with the relevant fire perimeter polygon. We spatially merge the perimeters with the school district map to determine, for each district-day, what fraction of the school district area was under active wildfire. For days falling within the fire interval (between the incident start and containment dates), this fraction reflects the spatial overlap between the fire perimeter and the district boundary; for all other days, the fraction is set to zero.

effects (θ_t) to absorb common annual shocks. β^k captures the dynamic relationship between wildfire smoke and the likelihood of school closures around smoke events on weekdays. Equation (7) is weighted by district level school enrollment and standard errors are clustered at the county level.

Student attendance in Texas. To study the impact of smoke on student attendance, we estimate equation (8). In contrast to the school closure data, the attendance data are reported at the annual level, so equation (8) investigates the causal relationship between the annual number of weekday smoke days and log of ADA. The main specification is a binned regression where bins are 0–1, 2–4, 5–9, 10–14, 15–19, and 20+ smoke days per school year (0–1 is the omitted category).[13]

$$\ln(\text{ADA}_{zt}) = \sum_k \beta_k \mathbf{1}[\text{Num. Smoke Days}_{zt} \in \text{Bin}_k] + X_{zt} + \delta_z + \gamma_t + \epsilon_{zt} \tag{8}$$

Equation (8) also controls for direct fire exposure X_{zt}, which is measured by the maximum fraction of school district area that was exposed to wildfire during the school year. We also include school district fixed effects (δ_z) and school-year fixed effects (γ_t). Similar to equation (7), regressions are weighted by district enrollment and standard errors are clustered at the county level.

Identification in both equation (7) and (8) leverage variation in wildfire smoke exposure, conditional on controls and fixed effects. Figure 1 illustrates that exposure to wildfire smoke (i.e., the annual number of weekdays with wildfire attributable $PM_{2.5}$ over 3 μg/m^3) varies substantially across years and space. This variation primarily reflects the plausibly random spatial and temporal distribution of wildfire location and the meteorological conditions such as wind direction and speed that govern smoke transport.

4.2 Impact of Wildfire Smoke on Mothers Time Use and Labor Outcomes

Next, we estimate the impact of wildfire smoke on mothers' time use and labor outcomes operating through disruptions in school-based childcare. Prior research shows that air pollution affects labor market outcomes through workers' own health, reducing labor supply or productivity (Aguilar-Gomez et al., 2022). To isolate the childcare channel from this biological channel, we compare how labor outcomes respond to wildfire smoke for mothers whose youngest child is aged 5–12 (elementary school age) versus mothers whose youngest child is either 0–4 or 13–17.

[13] The results are similar when we instead use a linear model, where the number of smoke days is the key regressor.

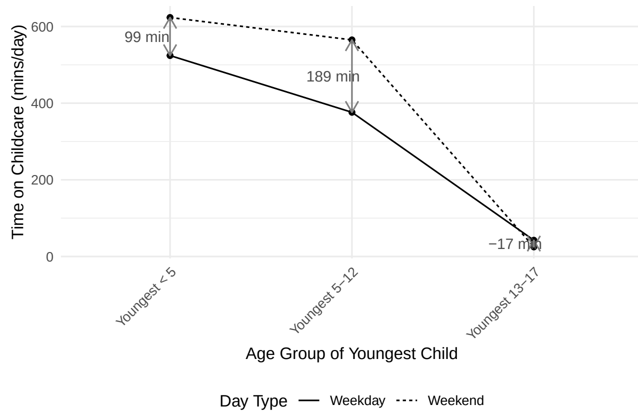


Figure 2: Childcare Time by Age of Youngest Child. This figure shows average childcare time for mothers by age group of the youngest child over the 2006–19 period. Childcare time is the sum of primary and secondary childcare time from ATUS, except for the youngest-child-aged-13–17 group, for whom secondary childcare time is not defined. Averages are calculated using ATUS weights.

This choice is guided by baseline time use patterns. Figure 2 plots average time spent on childcare (primary + secondary) by age of the youngest child and by day type (weekday versus weekend). Several patterns emerge. First, average time spent on childcare is substantially higher on weekends than on weekdays—a gap that narrows to near zero and reverses sign once the youngest child exceeds age 13, consistent with older children's greater self-sufficiency. Second, the difference in childcare time between weekdays and weekends varies substantially by the age of the youngest child. Specifically, the gap is 189 minutes for mothers whose youngest children are aged 5–12, compared to 99 minutes for those with younger children (0–4), likely reflecting differences in public schooling eligibility.

These patterns confirm that mothers with youngest children aged 5–12 are the group most likely to be affected by smoke-driven disruptions in school-based childcare. That is, smoke effectively converts weekdays into weekends most sharply for this group (i.e., $\frac{\partial B}{\partial N}$ in equation (5) is largest here). The comparison holds for student absenteeism due to illness as well: even if the probability of illness is identical across different age groups, the marginal increase in the caregiving burden is largest for mothers of elementary-school children who rely most heavily on weekday school-based care.

$$Y_{icsmt} = \beta_1 N_{csmt} + \beta_2 D_i + \beta_3 (N_{csmt} \times D_i) + \mathbf{X}_i + \delta_{cm} + \gamma_{st} + \epsilon_{icsmt} \tag{9}$$

Equation (9) formalizes the identification strategy discussed above. To fix ideas, first consider the contemporaneous effect of wildfire smoke. Y_{icsmt} represents labor outcomes such as absenteeism or hours worked during the reference week for individual i in area c, state s, and month–year mt. Our key regressor is $N_{csmt} \times D_i$, an interaction term of the number of wildfire smoke days ($PM_{2.5} > 3\mu g/m^3$) during the weekday of the reference week and an indicator for "treated" mothers, namely, mothers whose youngest child is aged 5–12. Thus, our coefficient of interest, β_3, captures the differential effect of wildfire smoke exposure on labor outcomes among mothers most dependent on school-based childcare relative to other mothers. In equation (9), we also include area-by-month fixed effects (δ_{cm}) to control for local seasonality and state-by-year fixed effects (γ_{st}) to absorb statewide shocks. We also control for individual level covariates ($\mathbf{X}_i$) such as age, education, industry, and race.[14] Because our contemporaneous analysis focuses on short-term disruptions in work due to wildfire smoke, we exclude individuals who are not employed at the time of the survey.

We complement the contemporaneous labor outcomes analysis with the ATUS data. While the identification strategy remains conceptually identical, we adapt equation (9) by replacing N_{csmt} with $Smoke_{csdt}$, an indicator equal to one if the respondent's diary day (dt) falls within the "smoke window." This adjustment reflects the temporal structure of the ATUS, which documents activities for a single reference day rather than a week. In practice, we define the smoke window as the period from one day before to four days after a smoke day, corresponding to the range with heightened school-closure probability reported in Figure 3a. Because the ATUS sample is roughly two orders of magnitude smaller than the CPS sample, we use a more parsimonious set of fixed effects (area, month, and year).

Finally, we also estimate the cumulative impact of wildfire smoke on labor outcomes using equation (9) with an important difference: N_{csmt} is the number of wildfire smoke days over the past 12 months, a time window that ensures all observations have a comparable exposure horizon in expectation.[15] Given the seasonality of wildfire smoke (Appendix Figure A.1), shorter windows (e.g., 3 or 6 months) may conflate smoke exposure with the season in which the CPS interview was conducted. Further, we do not control for industry because, unlike in the contemporaneous model where industry switching is unlikely, cumulative exposure may affect the industries in which individuals work.

[14] Equation (9) does not include controls for direct wildfire exposure. As discussed in Section 4.1, direct fire exposure is rare even in California, which is one of the most wildfire-active areas in the country. Only 0.8% of the observations in the school closure sample have any wildfire overlap, and excluding fire controls leaves estimates unchanged. Moreover, the CPS/ATUS sample excludes observations from sparsely populated areas, which are most likely to overlap with active fire perimeters (the so-called wildland-urban interface). Thus, direct fire exposure is likely lower than the 0.8%.

[15] In Section 5, we show that our results are robust to the inclusion of smoke exposure from more distant periods.

The key identification assumption for equation (9) is that, in the absence of differential reliance on school-based childcare, mothers' labor market responses to wildfire smoke would have been similar across groups. That is, the impact of wildfire through the own health channel or the impact of local labor demand would have been similar for two different groups of mothers. While this assumption is not directly testable, we assess its plausibility through a series of placebo tests and sensitivity analyses reported in Section 5.

Importantly, in running these regressions, we exclude mothers working in the elementary and secondary schools, as their responses to smoke-driven school closures (e.g., absenteeism or time use) are likely mechanical. Our regressions are weighted by relevant CPS or ATUS weights and standard errors are clustered at the area level.

5 Results

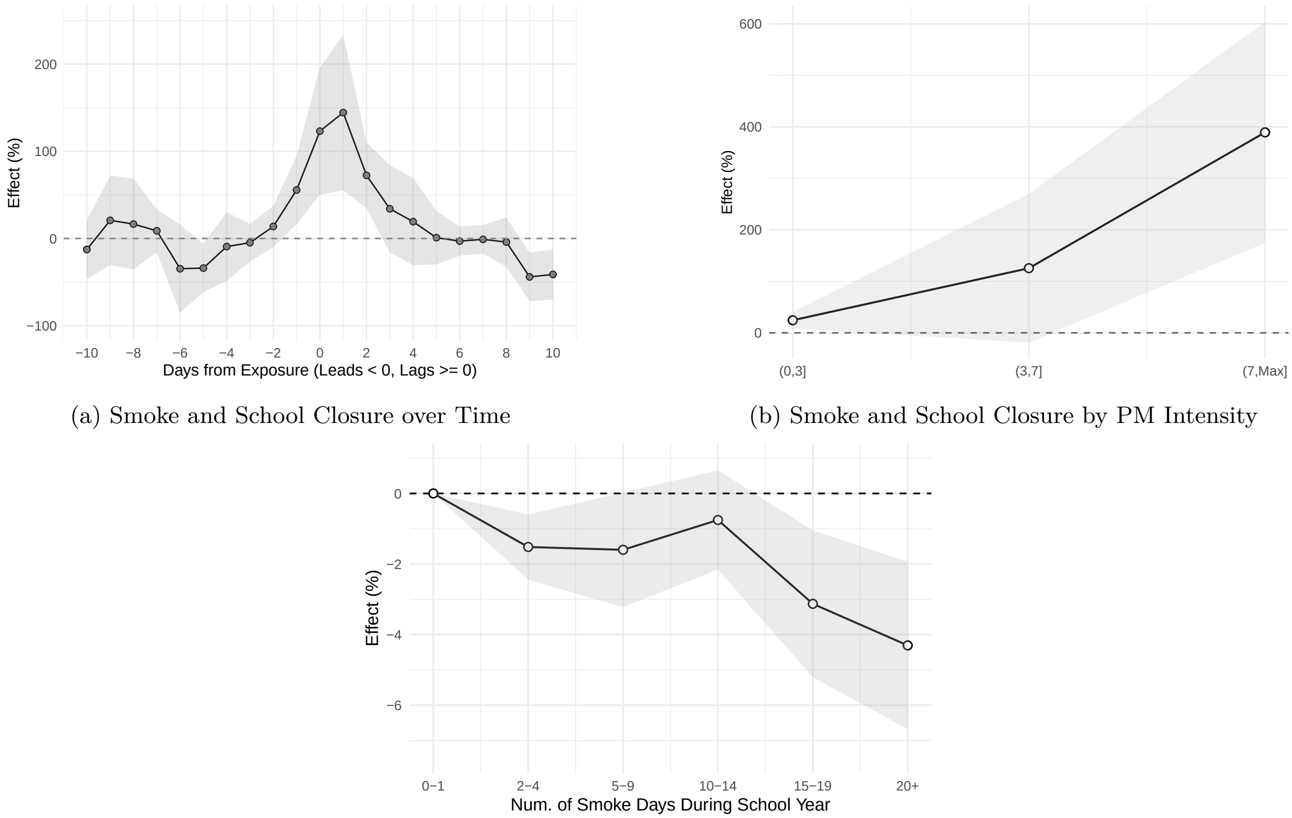
5.1 Contemporaneous Impacts of Wildfire Smoke

We begin by examining the first link in equation (5), namely whether wildfire smoke exposure translates into caregiving burden ($\frac{\partial B}{\partial N} > 0$) by investigating the impact of smoke exposure on school closures, student attendance, mothers' time use, and workplace absenteeism.

Impact on school closures and student attendance. Figure 3a reports the estimated coefficients from equation (7), capturing the dynamic effects of wildfire smoke on the likelihood of school closure during the 10 days before and after a smoke day. We find that a smoke day increases the probability of any school closure at the school district level by 150% on the day of the event. Moreover, there is substantial temporal spillover: the probability of closure begins to rise one day before the smoke day and remains elevated for up to four days afterward. This pattern likely reflects the strong serial correlation in day-to-day wildfire smoke exposure within locations.[16]

Next, we estimate a binned regression of daily closure probability on smoke intensity, using no-smoke days (wildfire-driven $PM_{2.5} = 0\mu g/m^3$) as the omitted category. Figure 3b shows that even relatively modest smoke exposures of $PM_{2.5}$ 3–7$\mu g/m^3$ lead to substantial increases in the probability of school closure, and the effects rise monotonically with smoke intensity. For days with wildfire-

[16] The average correlation between $PM_{2.5}$ on consecutive days within the same area is 0.75.



(a) Smoke and School Closure over Time

(b) Smoke and School Closure by PM Intensity

(c) Smoke and Student Attendance

Figure 3: The Impact of Wildfire Smoke on School Closures (CA) and Student Attendance (TX). Panel (a) plots the estimated coefficients from equation (7), and Panel (b) plots coefficients from a binned regression model with bins based on the daily wildfire-attributable $PM_{2.5}$ level. Both models are estimated using California school closure records. Panel (c) reports the estimated coefficients from equation (8) using the annual average daily attendance data from Texas. These regressions are weighted by enrollment size, and grey areas indicate 95 percent confidence intervals based on standard errors clustered on county.

driven $PM_{2.5} > 7, \mu g/m^3$, the estimated effect corresponds to an increase in closure probability of nearly 400% relative to no-smoke days. Given a baseline closure probability of 3.6% on no-smoke days, this implies a closure probability of 18% on high-smoke days.

In Figure 3c, we turn to the impact of smoke on student attendance, which allows us to gauge the increase in caregiving burden due to smoke even in the absence of school closure. The binned regression of annual attendance on smoke frequency reveals that attendance generally decreases as smoke frequency increases: relative to the omitted bin (0–1 smoke days), average daily attendance is 1.5% (1.7%) lower when a school year has 2–4 (5–9) weekday smoke days. There is a non-monotonic uptick at 10–14 days (around −0.8%), but the overall downward sloping trend is evident with ADA reduced by 3.2% at 15–19 days and 4.5% at 20+ smoke days. Given that ADA documents attendance on a normal school day (Texas Education Agency, 2024), this reduction in ADA in response to smoke days is consistent with the interpretation that smoke is making children sick enough to miss school (Currie et al., 2009).

While these results establish that wildfire smoke disrupts school operations and increases child illness, they do not directly speak to what happens to mothers during these events. To address this, we turn to the ATUS and CPS, which allow us to study mothers' time use and short term labor supply over the smoke period. Also, because these data are nationally representative, they allow us to assess whether these patterns extend beyond California and Texas.

Impact on mothers' time use. Smoke-driven caregiving shocks may affect how employed mothers allocate their time. In Table 1, we examine changes in time spent on childcare and paid work using ATUS data, comparing "treated" mothers—those whose youngest child is aged 5–12 (elementary school age)—to "control" mothers whose youngest child is aged 0–4.[17] To define the period affected by smoke exposure, we draw on the temporal spillover pattern observed in Figure 3a and classify any day within one day before and four days after a smoke day as part of the "smoke period."

Table 1 column (1) indicates that weekday smoke exposure increases total childcare time for treated mothers by 63 minutes (or 17%). As shown in columns (2) and (3), this increase is almost entirely driven by a rise in secondary childcare (i.e., supervising children while multitasking) rather than primary childcare. This pattern is consistent with the nature of childcare for elementary school children. Namely, when caregiving shocks arise, mothers of elementary school–aged children must provide care, but because these children are relatively independent, much of that care occurs alongside other tasks rather than as dedicated childcare.

Column (4) shows a 31-minute decline in working time, consistent with increased caregiving demands. Although the estimate is not statistically significant, the reduction in work time is roughly half the increase in childcare time, suggesting that the additional caregiving burden is absorbed through a combination of reduced work, leisure, and other activities. In contrast, columns (5)–(8) show that wildfire smoke that occurs on weekends does not lead to an increase in childcare time, suggesting that the weekday effects are primarily driven by disruptions to school-based care rather than through a direct effect of smoke on mothers themselves.

[17] Since the secondary childcare time variable is not defined for children older than 13, we restrict the ATUS analysis to mothers whose youngest child is aged between 0 and 12.

Table 1: Wildfire Smokes and Mothers' Time Use (Given Working)

	Weekdays: Yes				Weekdays: No			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var. (Mins/Day):	Total Childcare	Primary Childcare	Secondary Childcare	Working	Total Childcare	Primary Childcare	Secondary Childcare	Working
$PM_{2.5} > 3$ Period	-47.37**	-22.65**	-24.71	38.11*	26.60	-13.62	40.22*	2.769
	(18.90)	(9.130)	(16.08)	(19.64)	(20.99)	(10.73)	(21.82)	(16.57)
Youngest 5-12	-70.27***	-52.81***	-17.46**	33.94***	-18.76	-69.51***	50.75***	2.786
	(9.213)	(4.166)	(7.067)	(8.987)	(11.42)	(3.985)	(12.16)	(8.177)
$PM_{2.5} > 3$ Period x Youngest 5-12	62.53**	9.645	52.88**	-30.90	-31.94	0.9169	-32.86	-5.123
	(24.72)	(12.06)	(21.78)	(25.18)	(30.86)	(11.54)	(31.18)	(20.78)
Dep.Var Mean	361	114	247	398	556	95	461	114
Observations	6,076	6,076	6,076	6,076	6,420	6,420	6,420	6,420

Note:
This table reports the effects of wildfire smoke exposure on employed mothers' time use using the American Time Use Survey data from 2006–19. All specifications include area, year, and month fixed effects and controls for age, race, education, and industry. Standard errors clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Impact on contemporaneous labor outcomes. Smoke-driven disruptions in school-based childcare are likely to disrupt mothers' work. In Table 2, using CPS data, we estimate the effect of wildfire smoke exposure on work absenteeism and hours worked using equation (9) among employed mothers.

In column (1), we show that an additional weekday with wildfire smoke during the CPS reference week reduces total hours worked over that week by about six minutes (0.3%) for treated mothers relative to control mothers. To contextualize the magnitude, this is equivalent to 1 in 10 affected mothers working an hour less. In column (2), the probability of work absenteeism during the reference week increases by 0.0015 (3.7% relative to the baseline rate of 0.041). Given an average of 34.7 hours worked per week, the absenteeism coefficient of 0.0015 implies that full-week absences account for roughly $0.0015 \times 35 \approx 3$ minutes of the 6-minute reduction in hours worked. The remaining 3 minutes likely reflect adjustments among mothers who are not fully absent but work less—either missing one or two days within the reference week, which does not register as absenteeism in the CPS, or arriving late, leaving early, or otherwise reducing hours while remaining at work.

It is also worth noting that even though mothers can adjust along multiple margins (e.g., paying for emergency care or relying on social networks), smoke-driven caregiving shocks still lead to observable labor supply reductions on average, suggesting that for some mothers—particularly those with fewer adjustment margins—the disruption may be substantially larger. Moreover, the estimates in Table 2 likely understate the full scope of work disruption, as the CPS captures only formal absence and hours, ignoring disruptions while at work such as distraction or time spent arranging emergency childcare (Adams et al., 2025). These hidden costs are real but invisible in our data, yet visible to colleagues and supervisors, and may generate consequences that extend beyond the immediate disruption, a point we return to in the cumulative analysis.

In Appendix Table A.2 we reproduce Table 2 for employed fathers. We find that one additional smoke day during the CPS reference week increases fathers' absenteeism by 0.0012, but has no discernible impact on hours worked (if anything, the point estimate is slightly positive). These patterns suggest that while some fathers do help to absorb short-term caregiving demands by missing work, the adjustment is too limited to meaningfully offset the burden falling on mothers.

Table 2: Effect of Wildfire Smoke on Mothers' Contemporaneous Labor Outcomes (Given Working)

	(1)	(2)
Dependent Var.:	Hrs Worked	Absent
N $PM_{2.5} > 3$ (Weekly)	0.0523	-0.0011*
	(0.0366)	(0.0007)
Youngest 5-12	0.4807***	-0.0147***
	(0.0515)	(0.0006)
N $PM_{2.5} > 3$ (Weekly) x Youngest 5-12	-0.1019*	0.0015*
	(0.0566)	(0.0009)
Dep.Var Mean	34.742	0.041
Observations	1,049,006	1,049,006

Note:
This table reports the contemporaneous effects of wildfire smoke on employed mothers' hours worked and the probability of absenteeism during the CPS reference week. We use CPS monthly data from 2006–19 and restrict the sample to employed individuals. All models include area-month and state-year fixed effects, and controls for age, race, education, and industry. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

5.2 Cumulative Impacts of Wildfire Smoke

Main effects. Having established that wildfire smoke exposure generates caregiving burden by triggering school closures and student absenteeism, which increases childcare time and reduces hours worked for mothers, we next estimate the impact of cumulative smoke exposure on employment. In Table 3, column (1) shows that one additional weekday smoke day in the past 12 months reduces the probability of being employed by 0.0007 (or 0.11%). Importantly, as discussed in Section 2, this finding, combined with the evidence that $\frac{\partial B}{\partial N} > 0$, allows us to infer that higher caregiving burden raises the separation threshold w^R (i.e., $\frac{\partial w^R}{\partial B} > 0$), despite neither B nor w^R being directly observable.

Comparing columns (2) and (3) indicates that this reduction is concentrated in nonparticipation rather than unemployment. Likewise, columns (4) and (5) show that the decline in employment is concentrated among full-time positions, with little change in part-time work. Together, these results point to extensive margin adjustments consistent with mothers exiting full-time employment and leaving the labor force rather than substituting into part-time work.

Table 3: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$	0.0002	-6.81e-5	-0.0001	0.0002	2e-5
	(0.0002)	(7.21e-5)	(0.0002)	(0.0002)	(0.0001)
Youngest 5-12	0.0398***	0.0048***	-0.0446***	0.0268***	0.0130***
	(0.0033)	(0.0009)	(0.0031)	(0.0035)	(0.0018)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0007***	0.0001	0.0006***	-0.0006**	-0.0002
	(0.0002)	(8.21e-5)	(0.0002)	(0.0003)	(0.0002)
Dep.Var Mean	0.653	0.042	0.305	0.496	0.157
Observations	1,455,192	1,455,192	1,455,192	1,455,192	1,455,192

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes. We use CPS monthly data from 2006–19. All specifications include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

In Appendix Table A.3, we use CPS ASEC data to investigate whether lower employment probability in column (1) is explained by higher job exit and lower new entry. For this, we define an indicator for job exit that takes 1 when a mother not working in March of year t worked for more than 20 weeks in year $t-1$. Similarly, we define an indicator for job entry that takes 1 when a mother who worked less than 20 weeks in year $t-1$ is employed in March of year t.[18] The ASEC results indicate that the employment decline primarily reflects job exit rather than deterred entry: an additional weekday smoke day in the past 12 months increases the likelihood of job exit by 0.0003 (or 0.7% from the baseline of 0.046), while its effect on job entry is indistinguishable from zero.

Two additional features of the results also suggest that job exit is the primary driver. First, the contemporaneous results establish that smoke exposure generates immediate work disruptions among employed mothers, providing a natural precursor to eventual exit. Second, as we detail later, workplace characteristics are a strong determinant of the employment effect in column (1), which is hard to reconcile with the deterred entry effect.

How large is the effect size? An average mother in our sample experiences 8.6 weekdays with wildfire smoke per year, which implies roughly a 1% decline in employment probability among treated mothers. While modest in absolute magnitude, this effect suggests that unexpected caregiving shocks

[18] We used 10 or 30 weeks as alternative thresholds and find similar results.

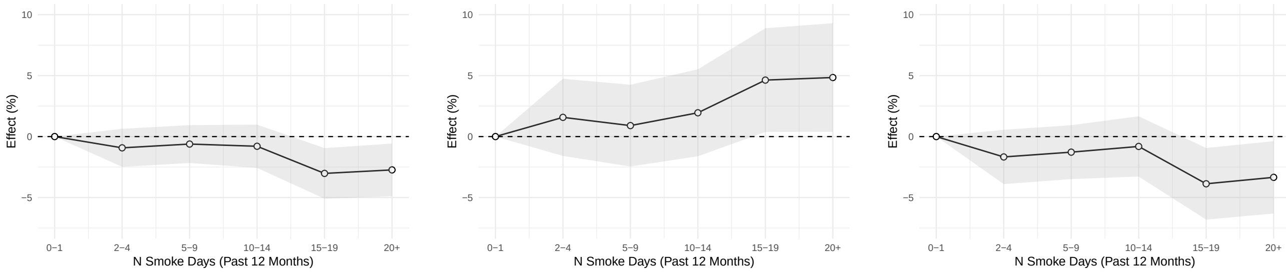
can meaningfully erode the maternal employment gains associated with institution-based childcare, a margin on which prior studies often find limited average effects (Cascio, 2009; Fitzpatrick, 2010; Goux and Maurin, 2010; Havnes and Mogstad, 2011; Fitzpatrick, 2012; Nollenberger and Rodríguez-Planas, 2015; Kleven et al., 2024).

Our estimates also speak to earlier findings on the impact of air pollution on labor outcomes. The magnitude of the caregiving channel appears comparable to that of the physiological channel: using a smoke day threshold consistent with Borgschulte et al. (2024), our estimates imply an annualized employment effect of roughly 0.044% per smoke day among treated mothers, similar to the 0.052% implied by their findings.[19] Our findings may also help rationalize sectoral patterns in Borgschulte et al. (2024), who document significant negative earnings effects in sectors such as health care, finance, and real estate. Yet they state that these effects are difficult to explain through direct physiological channels. In our sample, female shares in these industries are 79%, 54%, and 52%, respectively, suggesting the caregiving channel as a natural explanation.

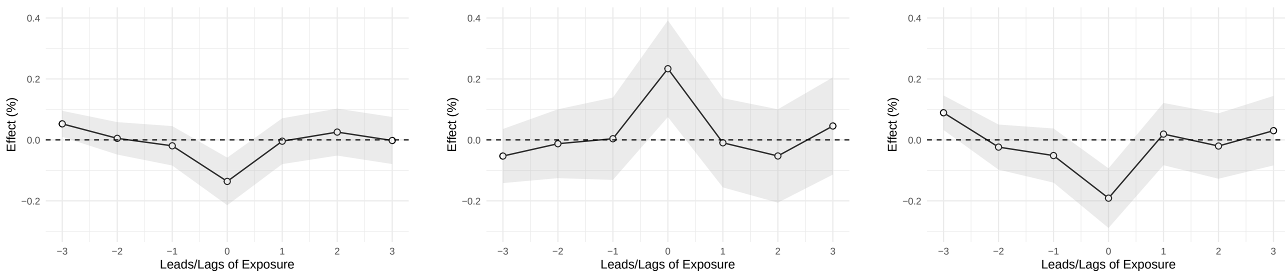
We next investigate three features of the dose-response relationship: how far back exposure matters, whether frequency or duration of exposure drives the effect, and whether the employment response is linear in exposure. We begin with the temporal horizon. In Appendix Table A.5, we augment the baseline specification by including smoke exposure from the past 13–24 and 25–36 months, along with their interactions with the treated group indicator, and find that conditional on exposure in the past 12 months, earlier exposure has a precisely estimated null effect on employment — consistent with the assumption in Section 2 that mothers' employment decisions respond to recent caregiving strain rather than more distant shocks.

We next ask which form of recent exposure matters for mothers' labor outcomes. For this, we decompose total exposure in the past 12 months into (i) the number of distinct smoke episodes and (ii) the average duration of those episodes, and interact each with the treatment group indicator. Appendix Table A.6 shows that even after controlling for smoke intensity, which is correlated with duration, repeated disruptions to caregiving arrangements, rather than the duration of any single episode, are the primary driver of the reduced employment. The finding accords with a simple intuition about how workplace relationships respond to repeated disruptions. A single episode that

[19]To facilitate comparison, we reproduce Table 3 using a smoke day threshold consistent with their definition—wildfire-driven $PM_{2.5}$ exceeding $1\mu g/m^3$—in Appendix Table A.4. The annualized figure from Borgschulte et al. (2024) is derived by applying simple linear scaling to their quarterly estimate of 0.013%.



(a) Binned: Employment (b) Binned: Not in Labor Force (c) Binned: Full-time Employment



(d) Dynamic: Employment (e) Dynamic: Not in Labor Force (f) Dynamic: Full-time Employment

Figure 4: The Cumulative Impact of Wildfire Smoke on Maternal Labor Supply. Panels (a)-(c) plot coefficients from a binned regression version of equation (9), while panels (d)-(f) plot coefficients from a version of equation (9) with three leads and lags of smoke exposure, both capturing the cumulative impact of wildfire smoke on maternal labor supply. Dependent variables are indicators for employment, non-participation in the labor force, and full-time employment. Grey areas represent 95 percent confidence intervals based on standard errors clustered at the area level.

forces a mother to miss an entire week of work is likely accommodated as an emergency—employers and colleagues rally around the affected worker, and the employment relationship remains intact. By contrast, repeated shorter disruptions scattered throughout the year with the same total missed days can create a pattern that, from the employer's perspective, signals underlying unreliability.

Finally, in Figures 4a–4c, we investigate the shape of the dose-response function by relaxing the constant marginal effect assumption implicit in equation (9). We replace the continuous exposure measure with six indicator variables for the number of weekday smoke events in the past 12 months: 0–1, 2–4, 5–9, 10–14, 15–19, and 20+. The estimated coefficients show that fewer than 15 smoke days does not meaningfully affect labor outcomes in comparison to the baseline category (0–1 smoke days). However, exposure exceeding two weeks leads to a sharp 3% decline in the probability of employment and full-time work, along with a more than 5% increase in non-participation, with effects stabilizing beyond 20 days. These patterns suggest a threshold in the dose-response relationship: modest caregiving disruptions can be absorbed without persistent labor market consequences, whereas sufficiently frequent shocks trigger discrete extensive-margin decisions.

Determinants of the effect size. Equation (6) implies that the employment effect should vary systematically with job and worker characteristics that shift either resource costs (k' in equation (6)) or workplace social capital (a' in equation (6)). In Table 4, we examine heterogeneity in β_3 in equation (9) along these two margins. In columns (1) and (2), we estimate equation (9) separately by whether a non-working adult resides in the household—a factor that can reduce the resource cost of caregiving shocks, as affected mothers may be able to rely on them for backup care.[20] The results indicate that the presence of a non-working adult does not mitigate the negative employment effect. Interestingly, in Appendix Table A.7, we find that the presence of an older adult (age 55 or above), who is a common source of informal childcare (Compton and Pollak, 2014), not only fails to mitigate the effect but worsens it—the negative impact of smoke on employment probability is more than twice as likely among these mothers. This likely reflects that older adults, while able to provide backup childcare, are among the most vulnerable to smoke-related health shocks and may themselves require additional care (Heft-Neal et al., 2023).

In columns (3) and (4), we estimate equation (9) separately by another determinant of k': occupational rigidity. In rigid occupations, wages are convex in hours worked (i.e., hourly wage is increasing in working hours), meaning foregone wages from reducing work hours due to caregiving shocks can be substantial (Goldin, 2014). To classify occupations by rigidity, we follow Goldin (2014) and use measures constructed from O*NET data. The results show that high occupational rigidity (the top tercile of the distribution in our sample) does not explain the reduction in employment probability: the point estimate for the high rigidity sample is virtually zero, whereas employment probability falls by 0.0004 for the low rigidity sample. Taken together, columns (1)-(4) indicate that higher resource costs do not appear to drive the employment effect of caregiving shocks.[21]

[20] In practice, these margins may interact. Access to backup care, which helps mothers remain at work on smoke days by lowering k', can reduce the need for workplace absences in the first place, thereby helping to preserve a. Importantly, while this may affect the *level* of a, it is less likely to affect the *slope* of a (i.e., a'). We therefore classify access to backup care as a factor that primarily affects resource costs (k' in equation (6)).

[21] Consistent with this, in Appendix Table A.7, we also find that state-level paid sick leave policies do not mitigate the employment effect.

Table 4: Effect of Cumulative Wildfire Smoke on Mothers' Prob. of Being Employed (Heterogeneous Effects)

	Resource Cost (k)				Workplace Social Capital (a)	
	Nonworking Adult in HH		Occ. Rigidity		Industry % Male	
	Yes	No	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var.:	Employed	Employed	Employed	Employed	Employed	Employed
N $PM_{2.5} > 3$	0.0004	0.0002	0.0002	8.44e-5	1.44e-5	0.0004**
	(0.0003)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0002)
Youngest 5-12	0.0133**	0.0507***	0.0012	-0.0016	-0.0021	0.0040
	(0.0062)	(0.0038)	(0.0020)	(0.0017)	(0.0019)	(0.0025)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0010*	-0.0007**	-0.0004**	3.16e-6	-6.47e-5	-0.0007***
	(0.0005)	(0.0003)	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Dep.Var Mean	0.641	0.658	0.919	0.964	0.935	0.933
Observations	381,970	1,073,222	665,300	348,575	687,460	343,749

Note:
This table reports the heterogeneous effects of cumulative wildfire smoke exposure on mothers' probability of being employed at the time of the CPS reference week. Each column presents estimates from equation (9) for different subsamples of CPS monthly data from 2006–19. All models include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

In columns (5) and (6), we use male industry share as a proxy for a', reflecting that male-dominated industries tend to have workplace cultures less tolerant of caregiving-related disruptions (Kleven et al., 2019). Indeed, the estimates show that the employment effect is entirely driven by mothers working in male-dominated (the top tercile of male industry share in our sample) industries: the employment probability falls by 0.0007 in response to one additional weekday smoke day in the past 12 months, whereas the effect is virtually zero for mothers outside male-dominated industries.[22]

A natural concern is that male-dominated industry may also be correlated with occupational rigidity from columns (3)-(4). We address this concern in Appendix Table A.8 by conditioning on occupational rigidity, and find that working outside of male-dominated industry offsets the employment effect within both high- and low-rigidity occupations, suggesting that it captures variation in workplace culture beyond what occupational rigidity alone explains.

If the contrast between columns (5) and (6) is indeed driven by workplace tolerance, we would expect mothers in more tolerant industries to absorb smoke-driven shocks through intensive margin adjustments—reducing hours and increasing absenteeism during smoke periods. To test this, we return to the contemporaneous framework and estimate the impact of weekday smoke events on hours worked and absenteeism during the same week separately by whether mothers work in male-dominated industries. Consistent with this prediction, in Table 5 columns (1) and (2), we find that smoke exposure reduces hours worked by 10 minutes and increases absenteeism by 0.2 percentage points among mothers outside of male-dominated industries. However, columns (3) and (4) suggest that the corresponding effects in male-dominated industries are small and statistically indistinguishable from zero. This pattern suggests that tolerant workplaces effectively provide an insurance mechanism: by accommodating short-run work disruptions through intensive margin adjustments, they prevent the gradual erosion of the employment relationship that ultimately drives job exit.

The contrast between Table 4 columns (3)–(4) and columns (5)–(6) reveals that workplace flexibility has two distinct dimensions with very different implications: ex-ante flexibility (the structural design of jobs such as low occupational rigidity before a shock) and ex-post flexibility (the cultural tolerance of workplaces when disruptions arise). Our results show that only the latter meaningfully buffers the employment effect of caregiving shocks.

[22] Results for both occupational rigidity and male industry share are robust to using the top quartile (top 25%) as an alternative cutoff. If anything, we find stronger magnitudes.

Table 5: Effect of Wildfire Smoke on Hours Worked and Absenteeism (By Industry Male Share)

	Industry % Male: Low		Industry % Male: High	
	(1)	(2)	(3)	(4)
Dependent Var.:	Hrs Worked	Absent	Hrs Worked	Absent
N $PM_{2.5} > 3$ (Weekly)	0.0875*	-0.0017**	-0.0230	9.45e-5
	(0.0480)	(0.0008)	(0.0646)	(0.0011)
Youngest 5-12	0.4711***	-0.0147***	0.4643***	-0.0144***
	(0.0612)	(0.0006)	(0.0881)	(0.0010)
N $PM_{2.5} > 3$ (Weekly) x Youngest 5-12	-0.1650**	0.0019*	0.0060	0.0008
	(0.0708)	(0.0010)	(0.1006)	(0.0014)
Dep.Var Mean	34.201	0.04	35.843	0.041
Observations	701,832	701,832	347,174	347,174

Note:
This table reports the contemporaneous effects of wildfire smoke on employed mothers' hours worked and the probability of absenteeism during the CPS reference week by the industry level share of male workers. We use CPS monthly data from 2006–2019 and restrict the sample to employed individuals. All models include area-month and state-year fixed effects, and controls for age, race, education, and industry. Each column presents estimates from equation (9) for different subsamples. Standard errors are clustered at the area level.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Results based on occupation or industry classifications (i.e., columns (3)–(6)) should be interpreted with caution, however, as they capture only transitions from employment to unemployment and do not capture transitions out of the labor force due to CPS data limitations.[23]

Robustness checks. We assess the robustness of our findings in several ways. First, in Figures 4d–4f, we estimate a dynamic version of equation (9) as a placebo test. Specifically, we test whether smoke exposure occurring before or after year t predicts labor outcomes in year t.[24] We find that weekday smoke exposure over the prior 12 months (event year 0) has a substantial effect on labor outcomes, with magnitudes closely corresponding to those reported in Table 3. In contrast, the coefficients on future exposure (event years 1 to 3) are close to zero, indicating that smoke exposure does not have lasting impacts once conditioned on the current (event year 0) exposure. Similarly, coefficients on earlier exposure (event years -1 to -3) are small and statistically indistinguishable from

[23] This is because occupation or industry codes are not available for respondents who are no longer in the labor force.
[24] Following Borgschulte et al. (2024), we assign smoke exposure for the same area and calendar month in the years $t-3$, $t-2$, $t-1$, $t+1$, $t+2$, and $t+3$ relative to the current year.

zero, suggesting no evidence of differential pre-trends between treated and control mothers prior to smoke exposure.

Second, Appendix Figure A.2 examines the sensitivity of our results to alternative definitions of a smoke day. Specifically, we vary the threshold for wildfire-driven $PM_{2.5}$ concentrations, defining smoke days as those exceeding 1, 3, 6, and 9 $\mu g/m^3$, respectively. Consistent with the intuition that higher treatment intensity should generate larger effects, we find a monotonically increasing relationship between the stringency of the smoke day definition and the estimated impact. For example, the effect of one additional smoke day on employment is estimated at –0.06%, -0.11%, –0.14%, and –0.14%, respectively, across the four thresholds—all of which are statistically significant. A similar pattern holds for full-time employment and labor force participation as well.

Third, in Appendix Table A.9, we implement a placebo test using fathers. Consistent with the literature documenting limited paternal responses to caregiving shocks (Kleven et al., 2019; Hansen et al., 2024), we find precisely estimated null effects on employment, labor force participation, and full- and part-time employment probabilities. This strengthens the interpretation that the effects for mothers reflect increased caregiving demand arising from disruptions in school-based childcare due to the smoke rather than general labor market conditions or health effects of smoke.

Fourth, we address a seasonality concern. As discussed in Section 3, smoke is concentrated in summer, and Price and Wasserman (2025) document summer dips in mothers' employment due to school recesses. If mothers with elementary-school children exhibit a larger decline in summer employment regardless of smoke, the results in Table 3 may partly reflect differential seasonality by treatment group rather than the causal effect of smoke-induced disruptions. To address this, in Appendix Table A.10 we allow for differential seasonality by treatment status through the inclusion of treatment-by-month fixed effects in addition to the baseline model, and find results strikingly similar to those in Table 3. A related concern involves time-varying summer shocks that differentially affect mothers of elementary-school children and are correlated with smoke exposure. Such confounds are unlikely to pose a meaningful threat to identification, however, given our rich set of fixed effects and the plausibly exogenous variation in wildfire smoke exposure.

Lastly, Appendix Table A.11 shows that our findings are robust to an alternative definition of treatment, namely having *any* child aged between 5–12. Similarly, Appendix Table A.12 and A.13 shows that an alternative control group definition that restricts to mothers with youngest child aged

13–17 or aged 0-4 yields nearly identical results.

6 Discussion and Conclusion

This paper provides the first causal evidence that stochastic caregiving shocks generate persistent reductions in maternal employment. Exploiting plausibly exogenous variation in wildfire smoke exposure as a source of caregiving disruptions and comparing mothers whose youngest child is of elementary-school age to those with younger or older children, we document three main findings. Smoke-driven disruptions to school-based childcare impose immediate costs: weekday childcare time rises by over one hour among treated mothers, accompanied by increases in work absenteeism and reductions in hours worked. These disruptions accumulate into lasting labor market consequences, reducing employment probability by approximately 1 percent at mean exposure levels, with effects concentrated in full-time work and driven by labor force exit rather than transitions to unemployment. This employment effect is not explained by the costs of arranging backup care or occupational rigidity, but by the erosion of the employment relationship in workplaces that penalize work disruptions arising from intermittent caregiving demands. Working outside of male-dominated industries, where such tolerance is more common, fully offsets the effect.

These findings reframe a longstanding puzzle in the childcare literature. A large body of evidence documents small or null effects of childcare expansions on maternal employment, often attributed to substitution from informal care or preferences for maternal care. Our results point to a distinct margin: even when formal care is available, its reliability remains consequential. Policies that move children across the threshold into formal care may relax one constraint on maternal employment while making another more salient: whether that care can be relied upon with sufficient regularity. Testing this interaction directly in the context of childcare expansions is beyond the scope of our analysis, but the results suggest that availability and reliability should be viewed as complementary margins rather than separate policy questions.

This workplace penalty extends and deepens Goldin (2014)'s account of the gender earnings gap to encompass the unpredictable demands that caregiving places on time. While our empirical setting is wildfire smoke, the mechanism is not specific to environmental disruptions. Any sufficiently frequent source of unpredictable caregiving demand, from a school closure triggered by a safety concern to a child's unexpected mental health episode, can generate employment effects of the same form in workplaces that penalize intermittent absence.

Stochastic caregiving shocks are not rare events. They are an unavoidable feature of raising children, and the costs they generate likely extend well beyond the immediate employment effects we estimate, with employment gaps generating earnings losses that persist long after the caregiving years have passed (Kleven et al., 2019; Cortés and Pan, 2023). That fathers absorb so little of this burden, as our placebo results confirm, suggests that more equitable sharing of caregiving within the household could meaningfully reduce the costs that mothers bear. Institutional design can also play an important role in insuring against the unpredictable caregiving demands that no family can fully anticipate. Whether that is best accomplished through workplace cultures that absorb these risks internally or through dedicated care institutions, such as drop-in childcare centers, that provide such insurance directly remains an open and important question.

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A Additional Tables and Figures

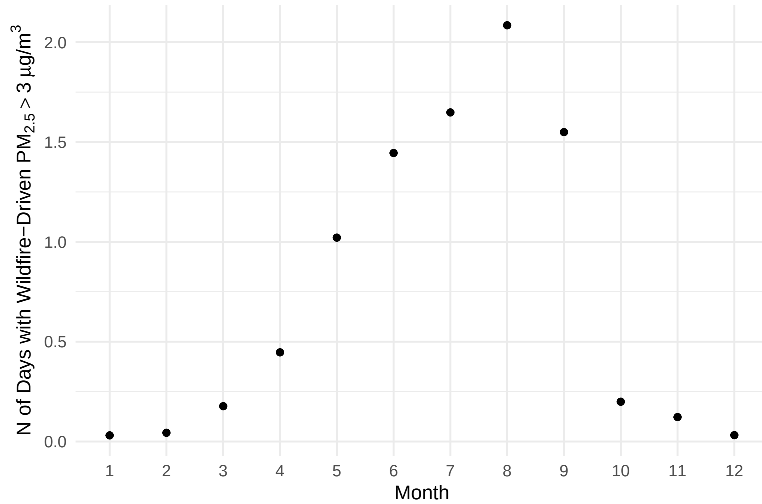

Figure A.1: Smoke Days by Month. This figure plots the average monthly number of days with wildfire-attributable $PM_{2.5}$ above $3\,\mu g/m^3$ from 2006-19 in areas included in the CPS sample.

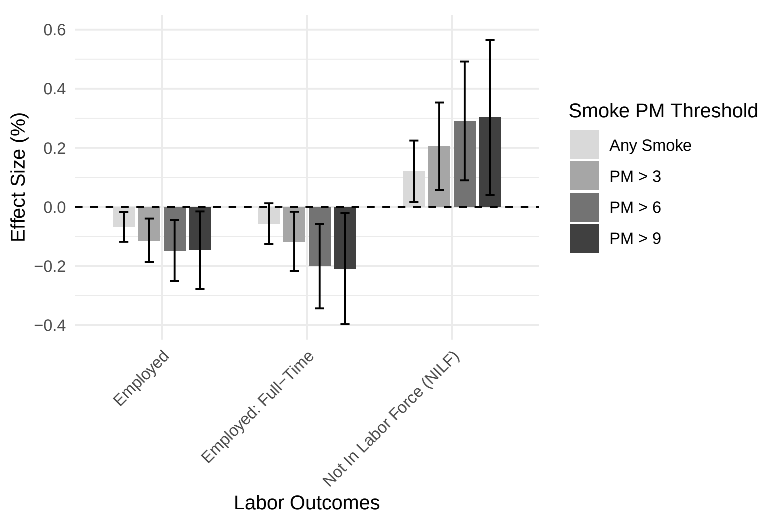

Figure A.2: Sensitivity Analysis. This figure plots the effect of cumulative smoke exposure on treated mothers using equation (9) based on alternative definitions (i.e., thresholds) of smoke days.

Table A.1: Descriptive Statistics

Variables	Min.	Max.	Median	Mean	Std.Dev.	N
Panel A: Monthly CPS						
Employed	0	1	1	0.653	0.473	1,455,192
Unemployed	0	1	0	0.042	0.199	1,455,192
Not In Labor Force (NILF)	0	1	0	0.305	0.456	1,455,192
Employed: Full-Time	0	1	1	0.496	0.5	1,455,192
Employed: Part-Time	0	1	0	0.157	0.369	1,455,192
Youngest: 0-4	0	1	0	0.365	0.48	1,455,192
Youngest: 5-12	0	1	0	0.422	0.494	1,455,192
Youngest: 13-17	0	1	0	0.213	0.412	1,455,192
N PM2.5 $>$ 3 (Past 12 Mon)	0	60	7	8.6	7	1,455,192
Hours Worked (Given Working)	0	168	40	34.7	13.7	1,049,006
Absenteeism (Given Working)	0	1	0	0.041	0.198	1,049,006
N PM2.5 $>$ 3 (Weekly)	0	5	0	0.147	0.561	1,049,006
Panel B: School Closures (CA) and Student Attendance (TX)						
Any School Closure	0	1	0	0.053	0.078	4,470,818
If PM2.5 $>$ 3	0	1	0	0.046	0.206	4,470,818
Average Daily Attendance	5.7	194,412	901	39,478	12,419	13,221
N PM2.5 $>$ 3 (Over School Year)	0	41	8	10.4	7.2	13,221
Panel C: ATUS (Unit: Mins/Day)						
Total Childcare Time	0	1,300	440	418	291	12,496
Primary Childcare Time	0	1,140	71	108	117	12,496
Secondary Childcare Time	0	1,290	315	310	273	12,496
Work Time	0	1,380	148	315	267	12,496

Table A.2: Effect of Wildfire Smoke on Fathers' Contemporaneous Labor Outcomes (Given Working)

	(1)	(2)
Dependent Var.:	Hrs Worked	Absent
N $PM_{2.5} > 3$ (Weekly)	0.0073	-0.0007*
	(0.0352)	(0.0004)
Youngest 5-12	0.1065**	-0.0016***
	(0.0450)	(0.0004)
N $PM_{2.5} > 3$ (Weekly) x Youngest 5-12	0.0125	0.0012**
	(0.0471)	(0.0006)
Dep.Var Mean	42.303	0.024
Observations	1,183,188	1,183,188

Note:
This table reports the contemporaneous effects of wildfire smoke on employed fathers' hours worked and the probability of absenteeism during the CPS reference week. We use CPS monthly data from 2006–19 and restrict the sample to employed individuals. All models include area-month and state-year fixed effects, and controls for age, race, education, and industry. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.3: Effect of Cumulative Wildfire Smoke on Mothers' Job Exit and Entry

	(1)	(2)
Dependent Var.:	Job Entry	Job Exit
N $PM_{2.5} > 3$ (Last calendar year)	8.84e-5	-0.0005**
	(0.0002)	(0.0002)
Youngest 5-12	-5.21e-5	-0.0046***
	(0.0015)	(0.0017)
N $PM_{2.5} > 3$ (Last calendar year) x Youngest 5-12	-8.49e-5	0.0003**
	(0.0002)	(0.0001)
Dep.Var Mean	0.041	0.046
Observations	235,138	235,138

Note:
This table reports estimates of the effects of cumulative wildfire smoke exposure on mothers' job entry and exit using CPS Annual Social and Economic Supplement data. All models include area and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.4: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Alt. Threshold)

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 1$	6.79e-5	-1.56e-5	-5.22e-5	9.27e-5	-2.49e-5
	(0.0001)	(5.39e-5)	(0.0001)	(0.0001)	(9.87e-5)
Youngest 5-12	0.0398***	0.0047***	-0.0445***	0.0259***	0.0139***
	(0.0035)	(0.0010)	(0.0034)	(0.0038)	(0.0021)
N $PM_{2.5} > 1$ x Youngest 5-12	-0.0004***	7.84e-5	0.0004**	-0.0003	-0.0002
	(0.0002)	(5.43e-5)	(0.0002)	(0.0002)	(0.0001)
Dep.Var Mean	0.653	0.042	0.305	0.496	0.157
Observations	1,455,192	1,455,192	1,455,192	1,455,192	1,455,192

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes. We use CPS monthly data from 2006–19. All specifications include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.5: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Longer Horizon)

	(1)	(2)	(3)
Dependent Var.:	Employed	Not In LF	Full-Time
Youngest 5-12	0.0376***	-0.0423***	0.0266***
	(0.0044)	(0.0040)	(0.0046)
N $PM_{2.5} > 3$ (Past 12 Months)	0.0002	-0.0002	0.0002
	(0.0002)	(0.0002)	(0.0002)
N $PM_{2.5} > 3$ (Past 13-24 Months)	0.0005**	-0.0004**	0.0004*
	(0.0002)	(0.0002)	(0.0002)
N $PM_{2.5} > 3$ (Past 25-36 Months)	0.0003	-0.0004*	0.0003
	(0.0002)	(0.0002)	(0.0002)
Youngest 5-12 x N $PM_{2.5} > 3$ (Past 12 Months)	-0.0007***	0.0006**	-0.0006**
	(0.0002)	(0.0002)	(0.0002)
Youngest 5-12 x N $PM_{2.5} > 3$ (Past 13-24 Months)	-4.79e-5	-1.39e-5	-4.78e-5
	(0.0002)	(0.0002)	(0.0002)
Youngest 5-12 x N $PM_{2.5} > 3$ (Past 25-36 Months)	0.0001	-0.0001	-5.11e-5
	(0.0003)	(0.0003)	(0.0003)
Dep.Var Mean	0.651	0.305	0.494
Observations	1,223,451	1,223,451	1,223,451

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes based on an alternative specification (including longer time horizon for smoke exposure). All models include area-month and state-year fixed effects, and controls for age, race, and education. We use CPS monthly data from 2006–19. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.6: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Duration vs. Episodes)

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$ Episodes	0.0003	-0.0002*	-9.4e-5	0.0004	-8.59e-5
	(0.0004)	(0.0001)	(0.0003)	(0.0004)	(0.0003)
Youngest 5-12	0.0390***	0.0053***	-0.0443***	0.0227***	0.0163***
	(0.0063)	(0.0020)	(0.0060)	(0.0067)	(0.0039)
Avg Duration of $PM_{2.5} > 3$ Episodes	-0.0015	0.0008	0.0008	-0.0023	0.0007
	(0.0028)	(0.0011)	(0.0028)	(0.0030)	(0.0019)
Avg $PM_{2.5}$ of $PM_{2.5} > 3$ Episodes	0.0086	-0.0028	-0.0058	0.0087	-0.0001
	(0.0109)	(0.0032)	(0.0110)	(0.0114)	(0.0062)
N $PM_{2.5} > 3$ Episodes x Youngest 5-12	-0.0013***	0.0004***	0.0009*	-0.0010**	-0.0003
	(0.0005)	(0.0002)	(0.0005)	(0.0005)	(0.0003)
Avg Duration of $PM_{2.5} > 3$ Episodes $\times$ Youngest 5-12	0.0007	-0.0012	0.0005	0.0043	-0.0036
	(0.0042)	(0.0014)	(0.0042)	(0.0044)	(0.0028)
Avg $PM_{2.5}$ of $PM_{2.5} > 3$ Episodes $\times$ Youngest 5-12	0.0014	-0.0003	-0.0011	-0.0167	0.0181
	(0.0153)	(0.0043)	(0.0157)	(0.0156)	(0.0124)
Dep.Var Mean	0.655	0.042	0.303	0.497	0.158
Observations	1,384,756	1,384,756	1,384,756	1,384,756	1,384,756

Note:
This table reports estimates of the effects of cumulative wildfire smoke exposure, decomposed into smoke duration and episodes, on mothers' labor outcomes during the CPS reference week. All models include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.7: Effect of Cumulative Wildfire Smoke on the Mothers' Prob. of Being Employed (by Older Adults and PSL)

	Adult 55+: No	Adule 55+: Yes	PSL: No	PSL: Yes
	(1)	(2)	(3)	(4)
Dependent Var.:	Employed	Employed	Employed	Employed
N $PM_{2.5} > 3$	0.0001	0.0007	0.0002	0.0008
	(0.0002)	(0.0005)	(0.0002)	(0.0005)
Youngest 5-12	0.0384***	0.0577***	0.0396***	0.0399***
	(0.0035)	(0.0087)	(0.0033)	(0.0088)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0007**	-0.0018**	-0.0007***	-0.0011*
	(0.0003)	(0.0008)	(0.0003)	(0.0006)
Dep.Var Mean	0.653	0.653	0.653	0.655
Observations	1,318,239	136,953	1,360,619	94,573

Note:
This table reports the heterogeneous effects of cumulative wildfire smoke exposure on mothers' probability of being employed at the time of the CPS reference week. Each column presents estimates from equation (9) for different subsamples. All models include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.8: Effect of Cumulative Wildfire Smoke on the Mothers' Prob. of Being Employed (by Male Share and Occupational Rigidity)

	Occ. Rigidity: Low		Occ. Rigidity: High	
	% Male: Low	% Male: High	% Male: Low	% Male: High
	(1)	(2)	(3)	(4)
Dependent Var.:	Employed	Employed	Employed	Employed
N $PM_{2.5} > 3$	3.79e-5	0.0005**	1.17e-5	0.0003
	(0.0002)	(0.0002)	(0.0001)	(0.0003)
Youngest 5-12	-0.0016	0.0058*	-0.0025	0.0018
	(0.0027)	(0.0032)	(0.0020)	(0.0036)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0001	-0.0008***	0.0001	-0.0004
	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Dep.Var Mean	0.917	0.923	0.966	0.957
Observations	432,597	232,703	245,438	103,137

Note:
This table reports the heterogeneous effects of cumulative wildfire smoke exposure on mothers' probability of being employed at the time of the CPS reference week. Each column presents estimates from equation (9) for different subsamples. All models include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.9: Effect of Cumulative Wildfire Smoke on Fathers' Labor Outcomes

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$	0.0003**	-0.0003***	-6.94e-5	0.0004***	-7.98e-5
	(0.0001)	(8.42e-5)	(9.64e-5)	(0.0002)	(8.05e-5)
Youngest 5-12	0.0022	0.0003	-0.0025*	0.0028	-0.0006
	(0.0019)	(0.0012)	(0.0014)	(0.0019)	(0.0012)
N $PM_{2.5} > 3$ x Youngest 5-12	-4.44e-5	0.0002*	-0.0001	-7.05e-6	-3.73e-5
	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
Dep.Var Mean	0.902	0.041	0.057	0.861	0.041
Observations	1,200,200	1,200,200	1,200,200	1,200,200	1,200,200

Note:
This table reports the effects of cumulative wildfire smoke exposure on fathers' labor outcomes. We use CPS monthly data from 2006–19. All specifications include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.10: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Controlling for Seasonality)

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$	0.0002	-6.84e-5	-0.0001	0.0002	2.02e-5
	(0.0002)	(7.21e-5)	(0.0002)	(0.0002)	(0.0001)
Youngest 5-12	0.1495	-0.2477	-0.2418	0.2772	0.3434
	(42.01)	(15.51)	(45.23)	(51.33)	(36.20)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0007***	0.0001	0.0006***	-0.0006**	-0.0002
	(0.0002)	(8.21e-5)	(0.0002)	(0.0003)	(0.0002)
Dep.Var Mean	0.653	0.042	0.305	0.496	0.157
Observations	1,455,192	1,455,192	1,455,192	1,455,192	1,455,192

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes while allowing for treatment group specific seasonality. We use CPS monthly data from 2006–19. All models include area-month, state-year, and treat-month fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.11: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Alt. Treatment Definition)

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$	0.0004*	-0.0002**	-0.0002	0.0005**	-0.0001
	(0.0002)	(8.33e-5)	(0.0002)	(0.0002)	(0.0002)
Any 5-12	-0.0296***	0.0018*	0.0278***	-0.0437***	0.0140***
	(0.0028)	(0.0010)	(0.0028)	(0.0034)	(0.0021)
N $PM_{2.5} > 3$ x Any 5-12	-0.0008***	0.0002**	0.0006**	-0.0009***	0.0001
	(0.0003)	(8.52e-5)	(0.0002)	(0.0003)	(0.0002)
Dep.Var Mean	0.653	0.042	0.305	0.496	0.157
Observations	1,455,192	1,455,192	1,455,192	1,455,192	1,455,192

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes based on an alternative treatment group definition (having any child aged 5-12). We use CPS monthly data from 2006–19. All specifications include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.12: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Alt. Control Definition)

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$	0.0002	-0.0002**	-1.71e-6	0.0004	-0.0002
	(0.0002)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Youngest 5-12	-0.0571***	-0.0010	0.0582***	-0.0796***	0.0224***
	(0.0039)	(0.0014)	(0.0037)	(0.0038)	(0.0030)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0006**	0.0003**	0.0004	-0.0008**	0.0001
	(0.0003)	(0.0001)	(0.0003)	(0.0003)	(0.0002)
Dep.Var Mean	0.694	0.043	0.263	0.536	0.158
Observations	929,292	929,292	929,292	929,292	929,292

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes based on an alternative control group definition (mothers with youngest child aged 13-17). We use CPS monthly data from 2006–19. All specifications include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.13: Effect of Cumulative Wildfire Smoke on Mothers' Labor Outcomes (Alt. Control Definition)

	(1)	(2)	(3)	(4)	(5)
Dependent Var.:	Employed	Unemp.	Not In LF	Full-Time	Part-Time
N $PM_{2.5} > 3$	0.0003	-7.85e-5	-0.0002	0.0001	0.0001
	(0.0002)	(8.88e-5)	(0.0002)	(0.0002)	(0.0002)
Youngest 5-12	0.1065***	0.0076***	-0.1140***	0.0984***	0.0080***
	(0.0035)	(0.0011)	(0.0033)	(0.0039)	(0.0020)
N $PM_{2.5} > 3$ x Youngest 5-12	-0.0009***	3.73e-5	0.0008***	-0.0005*	-0.0003**
	(0.0003)	(8.95e-5)	(0.0003)	(0.0003)	(0.0002)
Dep.Var Mean	0.632	0.043	0.325	0.472	0.16
Observations	1,139,695	1,139,695	1,139,695	1,139,695	1,139,695

Note:
This table reports the effects of cumulative wildfire smoke exposure on mothers' labor outcomes based on an alternative control group definition (mothers with youngest child aged 0-4). We use CPS monthly data from 2006–19. All specifications include area-month and state-year fixed effects, and controls for age, race, and education. Standard errors are clustered at the area level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.